and with a considerable saving of time and trouble. Once the position of the substage condenser is altered the whole system of illumination from the substage condenser to the source of light is displaced, which must again be adjusted, so that we have to effect centring at various points on the optical bench. This is entirely avoided by having the substage condenser supported in a fixed sleeve, and the centring screws attached to the objectives or the nosepiece.

As regards the draw-tube, the point is that the use of the mechanical draw-tube is advocated more in England than are the correction collars, but *vice versâ* on the Continent. When one is using an objective without correction collar, which is extremely sensitive to differences in thickness of cover glasses, it is not at all necessary to resort to a mechanical draw-tube if one's instrument is fitted with a carefully made simple draw-tube as is generally provided in the best Continental stands.

It may be mentioned, however, that still better results would be obtained by resorting to the use of a cover-glass gauge, an instrument which is very valuable and inexpensive, but rarely used by workers. In dealing with the standardisation of the microscope, the matter of cover glasses, which is a serious one, should also be considered with the view of providing microscopists with covers of much more uniform thickness.

Mr. Sutcliffe, in his letter which appeared in NATURE of January 18, asks if there is any evidence obtainable that the English instrument has lost its premier position. We have not far to seek the necessary information, and when the facts are known we cannot consider the English instruments "degenerate," as the present models are much superior to their predecessors. The best English microscopes are a credit to any well-organised manufactory. The Germans, however, have advanced further in the direction of providing more perfect instruments, which are produced in works better organised and with more highly scientific arrangements.

The instruments that combine in the highest degree simplicity, efficiency, and durability are the most valuable to the serious workers, and these features are strongest in the best Continental microscopes.

I do not believe any amount of discussion would bring us to a decision so satisfactorily as a close examination of instruments which have been in use for a number of years in the principal laboratories throughout the world. In fact, we need hardly look outside our own little island for the result, as there is plenty of material at home to deal with for a settlement of the dispute in a most practical manner.

J. W. OGILVY.

18 Bloomsbury Square, London, W.C., January 30.

The Mnemic Theory of Heredity.

IN a review of the third edition of Prof. Richard Semon's well-known book upon this subject, in NATURE of January 18 (p. 371), the reviewer writes as follows:—"The mnemic theory, which is based upon a belief in the inheritance of acquired characters, naturally does not appeal to those who deny the possibility of such inheritance." From the point of view of modern embryological research, both of these statements are open to challenge. The founder of this mnemic theory, or "memory as a general function of organised matter," has indeed written very little upon the subject, which he first broached in a public lecture in 1870. At the date named, when Prof. Ewald Hering, now of the University of Leipzig, gave his classic address, I imagine that the question of the inheritance or non-inheritance of acquired characters had hardly been raised. By a curious coincidence, it was in the same year that Prof. W. Waldeyer in his researches set up the doctrine of the somatic origin of germ-cells from the "germinal epithelium," and obviously this doctrine of the somatic or bodily origin of germ-cells is demanded by the view of an inheritance of acquired characters. The history of embryological research upon the germ-cells during the present century demonstrates clearly that in his researches in the lower vertebrates the writer first established—in 1900—an actual tangible continuity of germ-cells from generation to generation, and the absence of any genetic connection between Waldeyer's "germinal epithelium" and the germ-cells, that in many other cases these finds have been, and are still being, confirmed by the investigations of other observers, and that Prof. Waldeyer himself some years ago withdrew his former researches in favour of such a continuity of germ-cells as underlying the life-cycle. Indeed, he wrote:—"The consequences of this doctrine of the continuity of germ-cells are almost incalculable for every branch of biology"; so that now for animals a fundamental postulate of the doctrine of an inheritance of acquired characters has vanished.

But, as a fact, the inheritance of acquired characters is more a botanical doctrine than a zoological one. In plants this apparent inheritance of such characters is confined to *the asexual generations*, in which, if the term "individual" be used, it must apply to *all the products*, asexually produced, of one original plant—that is, in this sense they would all form a single individual. As an instance, in this way all the countless plants of the white chrysanthemum, Niveus, would form a single individual. The doctrine of an inheritance of acquired characters does not, and cannot, apply to *the sexual generations of animals*. Moreover, here it is refuted by the positive finds of embryology, which demonstrate, under an actual continuity of germ-cells from generation to generation, that nothing at all is handed on from parent to offspring. As this is the case, nothing acquired by the parent can be inherited. That is, there is in the sexual generations of animals no such thing as an inheritance of acquired characters.

I do not now remember when I first came to know of Prof. Hering's theory of heredity as based in the unconscious memories of germ-cells, but it must be a long time ago. As Samuel Butler remarks of Charles Darwin and the doctrine of natural selection, it must have become an "unconscious memory" to me, for in my researches into the history of the germ-cells I evolved it anew. As showing this, and as affording a simple statement of the main lines of the theory, I may quote the following,[1] written by me in 1904:—"From its nature it (the theory of heredity advocated by me) might be termed 'the Understudy-Theory of Heredity.' Given in a certain life-history the period of formation of the primary germ-cells. Of these let there be, for simplicity, but two, AB and BA. On one of these falls the lot of developing into an embryo: to which of the two this happens is not of consequence for the argument. In all its essential characters the remaining primary germ-cell (whose immediate destiny it is to become the founder of the 'sexual products' of the said embryo) is the exact counterpart of the developing one. So much so is this the case that, if both form embryos, these are identical twins. In the ancestry neither of the primary germ-cells, AB and BA, had ever been a higher animal; neither they nor their ancestors had ever formed part of the body of a higher animal. But their ancestry is continuous with a long line of germ-cells, and at regular intervals these were exactly like certain sister-cells, which did develop and form higher animal individuals. Although the cell AB does not itself give rise to an embryo, in the meantime it retains for itself, and also for all its immediate progeny, the properties of BA, those characters or potentialities which, were it or any of its progeny to develop, would make it or them identical twins with BA, the other cell which did develop. *This is the greatest wonder in embryology!* In the drama of heredity there are always understudies, which for a certain essential period are endowed with all the identical properties (potentialities) of that germ-cell from which the player arises. These understudies, the primary germ-cells, are never employed upon the stage as such—except in the instances of identical twins, triplets, &c.—but some of them in new guises and after new conjugations or unions are the immediate ancestors of those which become the acting characters in new scenes of the cyclical drama of Life."

The original German of Prof. Hering's lecture, like Austrian German in general, is difficult, and probably the original is little known in this country, although an excellent translation was published in Butler's "Unconscious

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[1] Beard, J., "A Morphological Continuity of Germ-Cells as the Basis of Heredity and Variation," in *Review of Neurology and Psychiatry*, vol. ii., 1904, p. 141.

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Memory" (1880, new edition 1910). To me, as an advocate of this theory of heredity, it comes as something new—and strange—that underlying it there should be the assumption of an inheritance of acquired characters. I would rather conclude that, like Francis Galton, this illustrious physiologist—with the "prevision" of which Pasteur so often spoke—foresaw that the individual was not at all "a chip of the old block," but that at the basis of all development there was a continuity of germ-cells. For on p. 17 of Ostwald's reprint of the original lecture Prof. Hering writes:—"From this point of view the whole individual development of a higher organised animal forms a continuous chain of memories of the development of that great series of beings whose final link this animal represents."

Like the late Samuel Butler, the writer rediscovered this theory of heredity, and except that author he was the first to advocate it, upon grounds of observation, in this country. As undoubtedly it is of all theories of heredity *the theory* which is capable of accounting for and explaining all the facts, I venture to ask the courtesy of the insertion of this brief account of it in your pages. In the light of this overwhelmingly important theory the "Mendelian discovery," for example, sinks into its proper place as a small but interesting episode in the history of heredity.
J. BEARD.

8 Barnton Terrace, Edinburgh, January 22.

I AM unable to agree with Dr. Beard that the mnemic theory of heredity does not involve acceptance of the doctrine of the inheritance of acquired characters. Certainly the theory as enunciated by Prof. Semon, which formed the subject-matter of my review, is based upon such acceptance, to justify which weighty evidence is brought forward. Can an organism, or a germ-cell, be said to remember events of which it has had no past experience, direct or indirect? If, as Dr. Beard holds, neither the primary germ-cells nor their ancestors have ever formed part of the body of a higher animal, can they be supposed to remember events in the ancestral history of the race, unless, of course, they have received information as to such events (engrams) from the bodies in which they are, or were, enclosed? The power of transmitting such engrams to the germ-cells is the fundamental conception of the doctrine of the inheritance of acquired characters, as it is also of the mnemic theory as expounded by Prof. Semon. If, however, Dr. Beard holds that the germ-plasm does not receive engrams from the body at all, but is merely a continuous stream of living matter which has the power of producing some particular type of body at intervals and under appropriate conditions, I fail to see where the idea of memory comes in, any more than in the case of the periodic waves produced by the tide.

I cannot see that the doctrine of the somatic or bodily origin of the germ-cells has any necessary connection with the doctrine of the inheritance of acquired characters. Even if we adopt the opposite doctrine, that the germ-cells form a continuous chain from generation to generation and are separated from the somatic cells at the very commencement of individual development, such a view does not seriously affect the question, for there is no valid reason for supposing that the germ-cells could be influenced by the somatic cells only through some protoplasmic connection.

Again, why should any distinction be drawn between plants and animals with regard to the problem under discussion? It would indeed be strange if the two great divisions of the organic world should differ in this respect. Of course, in the higher plants, the sexual generation (gametophyte) is very greatly reduced, but none the less a true sexual process intervenes between each asexual (sporophyte) generation and the production of the ripe seed. The case of the peach trees quoted in my review is in no way comparable to Dr. Beard's chrysanthemums, for the embryo plant within the seed is developed from a fertilised egg as truly as in the case of any animal. It is obvious, moreover, that in the case of the higher plants Dr. Beard's view as to the relations of the germ-cells cannot be maintained, for the whole sporophyte generation intervenes between each two successive sexual generations, and the latter develop each from a single non-sexual cell, the spore, produced by the sporophyte generation after it has attained maturity. Here, at any rate, there is no continuous chain of germ-cells distinguishable from somatic cells.

Dr. Beard's views on the subject of identical twins are new to me. I was under the impression that such twins were supposed to result from the complete division of a single fertilised ovum. This, at any rate, is the view adopted by Weismann.
ARTHUR DENDY.

The "Isothermal Layer."

I AM inclined to doubt whether Commander Hepworth's suggestion (NATURE, January 25) that the so-called "isothermal layer" is simply due to radiation from the material, solid or gaseous, which circulates round the sun with an orbital motion and gives rise to the zodiacal light, can be reconciled with the configuration of the surfaces of equal temperature in the upper air which show a progressive increase of temperature from low to high latitudes. It seems more probable that this increase, and the fact that above a certain height in these latitudes the temperature no longer diminishes with the altitude, are the result of the prevalent movement, outside the equatorial belt, of the higher portion of the atmosphere from west to east with comparatively great velocity, which increases with the latitude and altitude, and extends to lower levels as the distance from the equator becomes greater. This movement, which gives the upper atmosphere greater angular velocity than the lower and the earth beneath, partially counteracts the force of gravity and causes the air to rise and expand without doing work, and therefore without suffering a decrease in temperature. At the equator there appears to be no satisfactory evidence of an "isothermal layer."

But although the radiation from the orbital interplanetary matter of the zodiacal light may not afford an explanation of the "isothermal layer," it must be taken into account as a climatic factor. Maurer has shown that the earth receives at night radiated heat to the extent of 0·37 of a calorie per square centimetre per minute. This is attributed—no doubt in the main correctly—to radiation from the carbonic acid and water vapour of the atmosphere, but some portion must have an external source. It is possible that the radiation from interplanetary material may at present be almost as inconsiderable as that from the planets or the fixed stars; but if, as we have every reason to believe, there has been a gradual approximation of this diffused orbital matter towards, and absorption in, the sun, there must have been a time when so much was present beyond the earth's orbit that the radiation received from it balanced to a considerable extent the radiation from the earth into space, and rendered not only the daily and seasonal variations of temperature, but also the permanent differences of temperature between high and low latitudes, much less marked than they are at present.

I have for some time thought that it was in this direction we ought to look for the explanation of the comparative uniformity of temperature that appears to have prevailed in different latitudes in Palæozoic times, a uniformity that seems to have existed as much in periods of cold as of high temperature, and the absence of marked seasons even in the far north, evidenced by the fact that the remains of stems with exogenous growth show little or no trace of annual rings. In the long Arctic night, not only heat, but light, would have been continuously received from this source. How considerable, even at present, is the illumination given by the zodiacal light can only be realised by those who have travelled in moonless nights in the tropics. Even in forest country with a cloudy sky the darkness of midnight is changed about 2 a.m. to a twilight, which is quite sufficient to render the track visible until the true dawn appears.

Nordenskiöld has given reasons for believing that fine cosmic dust revolves round the earth itself as centre. If this be the case, its climatic influence in the past may have been similar, but it was probably of much less importance.
JOHN W. EVANS.

January 27.

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